MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXV. *May* 14, 1875. No. 7.

LORD LINDSAY, Vice-President, in the Chair.

Capt. Charles Orde Brown, late Royal Artillery, Plumstead House, Plumstead;

Percy Lyall Hooker Davis, Esq., Computer in the *Nautical Almanac* Office;

Henry Martyn Taylor, Esq., M.A., Fellow and Tutor of Trinity College, Cambridge, and

Sylvanus Phillips Thompson, B.A. (Lond.), St. Mary's, York,

were balloted for and duly elected Fellows of the Society.

Jabez Moden, Esq.. W. J. H. Beechey, Esq., and W. Hislop, Esq., were expelled from the Society for non-payment of arrears of subscriptions.

On the Dimensions of Venus as determined during the recent Transit. By Lieut.-Col. J. F. Tennant, R.E., F.R.S.

It is so very rarely that there is any possibility of the dimensions of *Venus* being satisfactorily measured, that I trust a determination, though in some respects imperfect, will be acceptable to astronomers.

The instrument used was a 6-inch telescope by Messrs. Cooke and Sons, equatoreally mounted, and supplied with a double-image micrometer, of Sir G. B. Airy's pattern, made by Messrs. Troughton and Simms. The micrometer was used during the Transit with the first lens, No. 3, and with this the magnifying power was about 128, and the value of one revolution of the screw was determined by a large number of transits of stars not far from the Pole to be $16''{\cdot}0625 \pm 0''{\cdot}0132$.

After the nearest limbs of *Venus* and the Sun had separated by four revolutions of the micrometer-head, I began to measure

diameters of the planet. At first I endeavoured to measure horizontal diameters, but I soon found these inconvenient, and I proceeded to measure diameters in Declination, and afterwards in Right Ascension. I had no suspicion that any ascertainable compression would be found, and consequently was not prepared to use exact position-angles; and, indeed, using the micrometer with the solar reflector interposed between it and the object, I could not have determined these without a great deal of time being spent on them. I believe the positions are, however, within two or three degrees of the truth, having placed the line of separation of the images so that one image followed the other when the appropriate slow motion was used.

This micrometer seems to me singularly convenient for such a purpose as measuring a distance, if greater accuracy could be got in determining the scale; and when it is used, as I have used it, with an instrument having convenient illumination and equatoreal motion, I believe this is attainable. In the first place, the thick wire used for taking the transits should be replaced by a fine one (silk fibre), and then this must admit of being moved in the eye-tube so that it may occupy the same position as the real image in the eye-tube. I have found the parallax most annoying even after I had endeavoured to correct it, and am quite confident that these improvements would be attended with great advantage.

At $14^h\ 05^m$ S.T. I began to observe the diameters of *Venus* in Declination. From a mean of 28 of these I obtained a double diameter $= 7^r.9339 \pm 0^r.007371$, the observations ending at $14^h\ 45^m$ S.T. The resulting diameter, in which is included a correction of $+\ 0''.070$ for refraction, is

$$\text{Diameter of } \textit{Venus} \text{ in Declination} = 63''.789 \pm 0''.0790.$$

Again at $15^h\ 10^m$ I began to observe diameters in Right Ascension, and 20 such observations were made up to $15^h\ 35^m$ of Sid. Time. From these I deduce the value of a double diameter in R.A. to be $7^r.9822 \pm 0^r.013496$, and the

$$\text{Diameter in R.A.} = 64''.132 + 0''.1205,$$

in which is included $0''.025$ for the effect of refraction.

At $15^h\ 36^m$ S.T. I again began a new set of observations in R.A., and completed 20 measures about $15^h\ 52^m$ S.T. The resulting double diameter was $7^r.9680 \pm 0^r.005963$, whence

$$\text{Diameter in R.A.} = 64''.015 \pm 0''.0711.$$

The mean of these two last is $64''.035 + 0''.0684$, which differs so considerably from the diameter in Declination as to give a decided indication of elliptic form. The value deduced for the compression is

$$c = \frac{1}{259.3 \pm 77.6}.$$

But it must be remembered that this result is an inferior limit to the value, as it is improbable that either the polar or equatoreal measures were taken. I do not know of any attempt to determine the inclination and node of the equator of *Venus*.

If we neglect the compression and combine the three results of diameter, we shall have

Mean Diameter of *Venus* as a sphere = 63″·948 ± 0″·0603.

These diameters, when reduced to the mean distance, give the following results :—

Declination (Polar?)	semidiameter	= 8″·4308 ± 0″·0010.
Right Ascension (Equatoreal?)	,,	= 8″·4633 ± 0″·0009.
Or Mean semidiameter as a sphere		= 8″·4518 ± 0″·0008.

The following previous determinations may be interesting, for comparison :—

Encke, from Transits of *Venus*	8″·305	Leverrier, *Annales*, vi. p. 26.
Airy, Measures on Meridian	8″·283	Do Do.
Main, D. I. Micrometer	8″·775	R. A. S. *Memoirs*, xxv p 46.
Stone, Measures on Meridian	8″·472	R. A. S. *M. N.* xxvi. p. 59.
Plummer, D. I. Micrometer	8″·661	Do. xxxiii. p. 561.

The value used in the *American Nautical Almanac* is 8″.546.

Roorkee Observatory,
1875, *March* 2.

Note on the Determination of the Scale in Photographs of the Transit of Venus.

By W. H. M. Christie, Esq.

In a paper in the *Monthly Notices* for March Capt. Abney has given reasons for concluding that photographs of the Sun taken by the English method (in which the primary image is enlarged) must be superior in point of definition both to those taken with lenses of very long focus and to daguerreotypes; but there remains the question—Whether there is any difficulty, peculiar to the first method, in determining the scale to which the distance between the centres of *Venus* and the Sun is referred? This question was naturally considered at an early stage of the British preparations; but I am not aware of any definite statement of the method available: and as the issue has been distinctly raised by Mr. Proctor, in a Note printed in the last number of the *Monthly Notices*, it may, perhaps, be worth while to explain a method which is applicable to all photographs by whatever method obtained, whether of the whole Sun or on a Janssen plate,

and which is equally available for heliometer or double-image measures.

In his Note on this subject Mr. Proctor has pointed out the sufficiently obvious objection that the Sun's diameter is increased by irradiation, whether optical or photographic, which must therefore enter as an unknown correction to the diameter, whether measured in a telescope or on a photograph; but whatever this unknown correction be, it is obvious that it must *diminish* the diameter of *Venus* to nearly, if not exactly, the same amount, so that the sum of the diameters of *Venus* and the Sun will only be affected by the difference of the irradiations. This is probably insensible, since in the English photographs a perfectly "flat" picture of the Sun has been obtained, so that the deposit of silver is of the same density at the limb as near the centre, whilst both *Venus* and the sky round the Sun are photographically black, giving absolutely no deposit. The question can, however, be settled by direct experiment, independently of any theory, for, the optical distortion of the photoheliograph being carefully determined from photographs of such a scale as Mr. De La Rue used at Kew, the breadths of the spider lines at different parts of the field may be calculated; or they may be found by direct measurement on a photograph of the sky, the photographic irradiation being in this case clearly the same all over the field. In any photograph of the Sun, then, it will only be necessary to compare the relative breadths of the spider line at the centre and limb of the Sun with the relative breadths as calculated, in order to obtain the difference of irradiation at the centre and limb.

This correction, then, being applied (if necessary), and also that for optical distortion, the ratio of the corrected distances of the centres of the Sun and *Venus* to the corrected sum of their diameters, as measured on the photographs in millimetres, expresses at once the ratio of the angular distance of the centres to the sum of the true angular diameters, the latter being two out of the five unknown quantities to be determined from all the observations of the Transit.

It is to be remarked that these measures are quite independent of any temperature correction; but in the case of the Janssen plates, only one limb of the Sun being included in the photograph, the scale corresponding to each temperature must be determined from the ordinary plates, and the measures of the diameter of *Venus* on each Janssen plate will then give the irradiation correction for that photograph to be applied to the Sun's limb. The distance of the planet from the limb in such plates being small, there will be no difficulty in determining the temperature correction with abundant accuracy.

It is clear that heliometer measures may be treated by a method generally similar to that applicable to the square plates, and double-image measures of cusps and limbs by that suggested for Janssen plates, the value of the screw being determined inde-

pendently from transits of stars in the case of the double-image micrometer. Thus, in all these cases, irradiation is to be treated as an unknown quantity, affecting all observations, except those of the true black drop, and to be eliminated by suitable treatment of the measures.

The question now arises whether any other method of treatment can be applied with advantage to photographs taken with lenses of long focus; and, in the absence of conclusive experiments bearing on this point, I may content myself with remarking that, the distance of the photographic plate from the optical centre of a forty-feet lens must be determined with a probable error not exceeding 1-9000th part, or 1-18th of an inch, in order to give the scale of the photograph with the same degree of accuracy as measures of diameters which are probably liable to an error of 1-300th of the diameter of *Venus*. Looking to the great difficulty in finding the optical centre of a lens of very long focus, especially when combined with a plane reflector, and the effect of temperature, it seems to me very doubtful whether a focal distance of 40 feet can be determined within 1-18th of an inch, while there is every hope that the probable error of measures of diameter (when made by the same observer throughout) will fall well within the limit assigned. These points, can, however, only be decided by actual experiment; but it is at least open to question whether the inherent difficulties in determining the distances of the primary focus and of the photographic plate from the optical centres of the object-glass, and of the enlarging combination, are really greater in the photoheliographs used by the English, Germans, and Russians, than in the long focus instruments used by the Americans. There is, besides, this important consideration that any error in the determination of the focal length will affect all the photographs taken with the same instrument, with a corresponding systematic error; whilst measures of diameter will only give rise to casual errors.

Blackheath, 1875, *May* 13.

Förteckning öfver Rektascensionerna för 103 *Fundamentalstjernor* (*List of the Right Ascensions of* 103 *Fundamental Stars*).

By Professor Hugo Gyldén: Stockholm.

(*Abstract by Mr. Wackerbarth.*)

Professor Gyldén's object in making the calculations here described was to obtain, for the reduction of his Stockholm Observations a system of fundamental stars, whose right ascensions were determined with all possible accuracy. He refers in the beginning of the essay to two similar works; the one by Professor Newcomb, carried out by that learned and accomplished writer with his usual skill, but with somewhat unsatisfactory

results, as his right ascensions are very generally not triflingly larger than those determined by the chief European Observatories, Greenwich, Pulkowa, and Paris, owing probably to his having assigned very great weight to the Washington Observations, and excluded those of Paris altogether. If we designate Professor Newcomb's results with N, and those of the following pages by S, the following table will give an idea of the discrepancies:—

	N−S s		N−S s		N−S s
α Andromedæ	+0·031	α Hydræ	+0·007	α Herculis	+0·020
γ Pegasi	+0·030	α Leonis	+0·011	α Ophiuchi	+0·021
α Arietis	+0·030	β Leonis	+0·006	α Lyræ	+0·045
α Ceti	+0·015	α Virginis	+0·009	γ Aquilæ	+0·026
α Tauri	+0·004	α Boötis	+0·027	α Aquilæ	+0·018
β Orionis	−0·018	α^2 Libræ	+0·011	β Aquilæ	+0·034
β Tauri	+0·001	α Coronæ	+0·028	α^2 Capricorni	+0·020
α Orionis	−0·009	α Serpentis	+0·012	α Aquarii	+0·034
β Geminorum	+0·004	α Scorpii	+0·009	α Pegasi	+0·018

where the regularity of the differences show that the individual right ascensions have been very accurately determined.

The other list is that printed in the *Vierteljahrsschrift* (Quarterly Journal) of the German Astronomical Society. It is based on Greenwich, Paris, and Pulkowa observations, and the individual right ascensions are determined with great accuracy. The equinoxial point is less certain, being based only on Pulkowa observations of 30 years ago. To reduce the observations to 1875, it was necessary, for obtaining the proper motions, to make use of Bradley's Catalogue of 1755, so that whatever error Bradley may have committed in his determination of the equinox will enter here with one-third of its amount. Nevertheless, Dr. Gyldén's calculations show, that these absolute right ascensions are remarkably near the truth. It is of course to be supposed, that this system will harmonize very closely with that of Pulkowa when the latter is reduced to 1875; and as such it is here denoted by P.

With this and with each other the determinations of Greenwich, Paris, and Washington have been compared as follows:—

1°, *Greenwich.* The Catalogues appended to the Greenwich Observations from 1865 to 1869 inclusively have been employed, and the positions reduced to 1875 with Bessel's Precessions, and Mädler's Proper Motions. The means of the results have been considered as the definitive Greenwich positions, and are distinguished here by the letter G.

2°, *Paris.* The Paris positions are taken from the *Annales de l'Observatoire de Paris*, 1863–7, reduced to 1875; the means of these being taken are here marked Pa. But as the equinoxial point is not determined in these volumes, the constant quantity

$0^s.016$, deduced from the Paris Observations 1856-9,* has been added to all the Pa. right ascensions; and the thus amended results called Pa_1.

3°, *Washington.* The Washington positions are those given in Professor Newcomb's Appendix III., to the Astronomical Observations of 1867, reduced, with the above-mentioned precession and proper motion, to 1875; and are here distinguished by the letter W.

The mean of the differences of these systems for each hour of right ascension being taken, † and the differences themselves being considered as small quantities whose products and higher powers may be neglected, a trigonometrical formula of interpolation in which terms with arguments exceeding 2α could be rejected was formed. The result is—

$$\begin{aligned}
G-P &= -0^s.002 - 0^s.009 . \cos\alpha - 0^s.007 . \sin\alpha - 0^s.001 . \cos 2\alpha - 0^s.002 . \sin 2\alpha \\
G-Pa_1 &= +0^s.005 + 0^s.002 . \cos\alpha - 0^s.015 . \sin\alpha - 0^s.002 . \cos 2\alpha - 0^s.002 . \sin 2\alpha . \\
G-W &= -0^s.045 - 0^s.012 . \cos\alpha + 0^s.009 . \sin\alpha - 0^s.014 . \cos 2\alpha + 0^s.001 . \sin 2\alpha .
\end{aligned}$$

Adding to these the identity, $G-G = 0$, and putting

$$S = \frac{1}{4}\{G+P+Pa_1+W\},$$

we obtain

$$G-S = -0^s.010 - 0^s.005 . \cos\alpha - 0^s.003 . \sin\alpha - 0^s.004 . \cos 2\alpha - 0^s.001 . \sin 2\alpha ;$$

which, combined with the preceding, gives

$$\begin{aligned}
S-G &= +0^s.010 + 0^s.005 . \cos\alpha + 0^s.003 . \sin\alpha + 0^s.004 . \cos 2\alpha + 0^s.001 . \sin 2\alpha . \\
S-P &= +0^s.008 - 0^s.004 . \cos\alpha - 0^s.004 . \sin\alpha + 0^s.003 . \cos 2\alpha - 0^s.001 . \sin 2\alpha . \\
S-Pa_1 &= +0^s.015 + 0^s.007 . \cos\alpha - 0^s.012 . \sin\alpha + 0^s.002 . \cos 2\alpha - 0^s.001 . \sin 2\alpha \\
S-W &= -0^s.035 - 0^s.007 . \cos\alpha + 0^s.012 . \sin\alpha - 0^s.010 . \cos 2\alpha + 0^s.002 . \sin 2\alpha .
\end{aligned}$$

The Melbourne Observations have been also taken into account, although these are not strictly absolute determinations, as they are based on the equinoxial point adopted in the *Nautical Almanac.* Denoting them with M, we have

$$\begin{aligned}
G-M &= +0^s.026 + 0^s.007 . \cos\alpha - 0^s.028 . \sin\alpha - 0^s.005 . \cos 2\alpha + 0^s.002 . \sin 2\alpha . \\
S-M &= +0^s.036 + 0^s.012 . \cos\alpha - 0^s.025 . \sin\alpha - 0^s.001 . \cos 2\alpha + 0^s.003 . \sin 2\alpha .
\end{aligned}$$

The right ascensions in the four systems G, Pa_1, W and M having been amended by means of the above formulæ, the separate means were taken, and the following right ascensions were the result:—

* *Vide* Nyrén's *Försök till Bestämning af Precessionskonstanten. Uppsala,* 1869, p. 30.

† The numbers in the *Vierteljahrsschrift* have been *here* employed only to ascertain the systematic differences of different lists.

	h	m	s
α Andromedæ	0	1	55·733
γ Pegasi	0	6	48·012
12 Ceti	0	23	39·565
β Ceti	0	37	18·830
ε Piscium	0	56	27·415
θ Ceti	1	17	46·536
η Piscium	1	24	47·765
υ Piscium	1	34	55·632
8 Arietis	1	47	44·232
α Arietis	2	0	7·773
67 Ceti	2	10	44·937
ξ Ceti	2	21	30·855
γ Ceti	2	36	49·486
α Ceti	2	55	44·779
δ Arietis	3	4	29·004
η Tauri	3	40	3·365
γ Eridani	3	52	11·849
o′ Eridani	4	4	45·872
ε Tauri	4	21	19·136
α Tauri	4	28	44·959
ι Aurigæ	4	48	51·305
ε Leporis	5	0	10·217
8 Orionis	5	8	31·880
β Tauri	5	18	23·464
δ Orionis	5	25	37·299
α Leporis	5	27	13·074
ε Orionis	5	29	52·278
α Orionis	5	48	24·291
ν Orionis	6	0	26·102
μ Geminorum	6	15	23·907
γ Geminorum	6	30	29·418
γ Canis Majoris	6	58	6·230
δ Geminorum	7	12	39·393
α Geminorum	7	26	37·297
β Geminorum	7	37	39·890
6 Cancri	7	55	50·323
η Cancri	8	25	28·692
ε Hydræ	8	40	9·339
83 Cancri	9	12	0·152
α Hydræ	9	21	26·80
ε Leonis	9	38	45·162
π Leonis	9	53	36·394
α Leonis	10	1	42·799
γ Leonis	10	13	4·687
ρ Leonis	10	26	13·662
l Leonis	10	42	41·136
χ Leonis	10	58	34·075
δ Leonis	11	7	27·507
δ Crateris	11	13	5·539
υ Leonis	11	30	32·912
β Leonis	11	42	40·957
η Virginis	12	13	30·647
γ^1 Virginis	12	35	19·585
12 Canum Venat.‡	12	50	10·680
θ Virginis	13	3	28·741
α Virginis	13	18	36·564
ζ Virginis	13	28	19·488
η Boötis	13	48	43·965
τ Virginis	13	55	17·146
α Boötis	14	9	57·606
ρ Boötis	14	26	26·573
ϵ^2 Boötis	14	39	31·665
α^2 Libræ	14	43	57·924
ψ Boötis	14	59	5·374
β Libræ	15	10	16·924
α Coronæ	15	29	23·740
α Serpentis	15	38	6·707
β^1 Scorpii	15	58	10·242
δ Ophiuchi	16	7	47·777
α Scorpii	16	21	44·721
ζ Herculis	16	36	34·514
κ Ophiuchi	16	51	45·101
α Herculis	17	8	56·886
θ Ophiuchi	17	14	20·025

‡ In years subsequent to 1870 this star is in the *Nautical Almanac* called α *Canum Venaticorum.*

	h	m	s
α Ophiuchi	17	29	7·939
μ Herculis	17	41	34·007
μ Sagittarii	18	6	17·250
α Lyræ	18	32	42·352
β Lyræ	18	45	27·877
ζ Aquilæ	18	59	39·858
ω Aquilæ	19	11	56·920
δ Aquilæ	19	19	11·715
h^2 Sagittarii	19	29	5·918
γ Aquilæ	19	30	18·999
α Aquilæ	19	44	41·052
β Aquilæ	19	49	10·360
a^2 Capricorni	20	11	7·070
ρ Capricorni	20	21	43·710
32 Vulpeculæ	20	49	13·953
61^1 Cygni	21	1	17·653
ζ Cygni	21	7	36·990
β Aquarii	21	24	58·644
ε Pegasi	28	38	2·782
16 Pegasi	21	47	22·498
α Aquarii	21	59	21·772
θ Aquarii	22	10	14·053
η Aquarii	22	28	55·943
ζ Pegasi	22	35	13·648
α Pegasi	22	58	32·102
γ Piscium	23	10	41·104
κ Piscium	23	20	31·486
ι Piscium	23	33	31·288
ω Piscium	23	52	53·558

These positions have a probable error of only $\pm 0^s\cdot007$, or when we include the systematic uncertainty arising from differences in the constant terms $\pm 0^s\cdot009$, so that their accuracy may be said to be $0^s\cdot01$.

The following is a comparison of them with the list in the *Vierteljahrsschrift*, the *Nautical Almanac*, and the *Berliner Jahrbuch*:—

Star.	S.−P.	S.−*N.A.*	S.−*B.J.*
α Andromedæ	+0·007	+0·004	+0·008
γ Pegasi	−0·012	−0·004	−0·080
12 Ceti	−0·025	+0·041	
β Ceti		+0·053	0·000
ε Piscium	+0·002	−0·038	
θ Ceti	0·000	+0·065	
η Piscium	−0·028	+0·014	
ν Piscium	−0·023	+0·063	
β Arietis	+0·004	+0·077	
α Arietis	−0·006	+0·018	−0·046
67 Ceti	−0·007	+0·043	
ξ^2 Ceti	+0·027	+0·028	
γ^2 Ceti	−0·005	+0·028	
α Ceti	+0·015	+0·032	−0·007
δ Arietis	−0·033	0·000	−0·117
η Tauri	+0·011	+0·008	
γ Eridani		+0·041	

Star.	S.–P.	S.–*N.A.*	S.–*B.J.*
o^1 Eridani	−0·025	+0·078	
ϵ Tauri	−0·002	+0·041	
α Tauri	+0·005	+0·007	−0·034
ι Aurigæ	+0·011	+0·028	
ϵ Leporis		+0·078	
β Orionis	+0·004	+0·054	−0·036
β Tauri	+0·004	−0·035	−0·028
δ Orionis	−0·023	+0·041	
α Leporis		+0·034	
ϵ Orionis	−0·013	+0·074	
α Orionis	+0.001	+0·011	−0·067
ν Orionis	−0·028	+0·003	
μ Geminorum	−0·002	−0·001	
γ Geminorum	−0·002	+0·018	
γ Canis Majoris		+0·012	
δ Geminorum	0·000	−0·039	
α^2 Geminorum		−0·056	+0·251
β Geminorum	+0·010	+0·005	−0·032
6 Cancri	+0·021	+0·051	
η Cancri	−0·010	+0·034	
ϵ Hydræ	+0·006	+0·021	
83 Cancri	−0·035	+0·042	
α Hydræ	+0·005	+0·040	−0·031
ϵ Leonis	0·000	−0·048	
π Leonis	−0·008	0·000	
α Leonis	+0·014	+0·001	0·015
γ^1 Leonis		−0·016	−0·025
ρ Leonis	+0·049	−0·051	
l Leonis	−0·004	+0·034	
χ Leonis	−0·020	−0·002	−0·124
δ Leonis	+0·036	−0·025	−0·137
δ Crateris		+0·012	−0·070
ν Leonis	+0·014	+0·040	
β Leonis	+0·015	+0·012	−0·028
η Virginis	+0·020	+0·032	
γ^1 Virginis	−0·032		
12 Canum Venat.	+0·035	+0·021	
θ Virginis	−0·003	+0·029	
α Virginis		+0·048	−0·023

Star.	S.–P.	S.–*N.A.*	S.–*B.J.*
ζ Virginis	+0·022	+0·002	−0·151
η Boötis	+0·010	−0·030	−0·149
τ Virginis	+0·018	−0·002	
α Boötis	+0·011	−0·024	−0·015
ρ Boötis	+0·013	−0·009	
ϵ^2 Boötis		−0·010	
α^2 Libræ		0·000	−0·023
ψ Boötis	−0·011	−0·030	−0·048
β Libræ	+0·001	+0·045	
α Coronæ	+0·001	−0·029	−0·050
α Serpentis	+0·018	+0·008	−0·027
β^1 Scorpii		+0·016	
δ Ophiuchi	−0·006	+0·070	
α Scorpii		+0·012	−0·010
ζ Herculis	+0·004	+0·017	−0·162
κ Ophiuchi	+0.037	+0·040	+0·019
α Herculis	−0·005	+0·002	−0·062
θ Ophiuchi		+0·022	
α Ophiuchi	+0·016	+0·040	−0·013
μ Herculis	−0·003	−0·005	+0·038
μ^1 Sagittarii		+0·056	
α Lyræ	−0·023	−0·020	−0·050
β Lyræ	+0·031	−0·009	−0·011
ζ Aquilæ	+0·021	+0·051*	
ω Aquilæ	−0·007	+0·028	
δ Aquilæ	−0·004	+0·054	+0·003
h^2 Sagittarii		+0·068	
γ Aquilæ	−0·013	−0·017	−0·046
α Aquilæ	−0·008	+0·009	−0·052
β Aquilæ	−0·012	+0·008	−0·062
α^2 Capricorni		+0·060	−0·027
ρ Capricorni		+0·064	
32 Vulpeculæ		−0·009	
61^1 Cygni	+0·006	+0·129	+0·128
ζ Cygni	−0·081	+0·026	
β Aquarii	−0·001	+0·071	

* The difference between the R.A. given and that of the *Nautical Almanac* is $0^s{\cdot}061$; but whether the misprint is here or in the list of Right Ascensions, I am unable to say.

Star.	S.–P.	S.–*N.A.*	S.–*B.J.*
ϵ Pegasi	+0·008	−0·040	
16 Pegasi	+0·008	−0·031	
α Aquarii	−0·006	+0·066	−0·052
θ Aquarii		−0·084	
η Aquarii	+0·028	+0·058	
ζ Pegasi	+0·019	+0·044	
α Pegasi	−0·021	+0·027	−0·039
γ Piscium	+0·016	+0·040	+0·018
π Piscium	−0·010	+0·050	
ι Piscium	−0·002	+0·042	−0·119
ω Piscium	+0·025	+0·006	−0·076

The errors of the press in Professor Gyldén's essay are very numerous; those which I have observed I have corrected, where I have had the means of doing so.

Uppsala, 1875, *May* 1.

On the Proper Motion of the Star in Cetus, marked 793 *in the British Association Catalogue.* By C. Piazzi Smyth, Astronomer Royal for Scotland.

In the course of preparing the forthcoming Edinburgh Catalogue of Stars (intended chiefly to illustrate Proper Motion and facilitate its further observation), the most interesting case that I have yet come across is the small 6·5 mag. star in *Cetus*, numbered 793 in the British Association Catalogue, and otherwise known as Piazzi II. 123; Taylor II. 268; W. 156; and Weisse II. 464.

It is interesting, not only from the proper motion being large in both elements of place, but from its undergoing undoubted variations with time; and also from its catalogue-history having chanced to render it a luminous example of the importance of the modern method of accompanying the one general date of the epoch of any Catalogue by the particular dates of the observations concerned with each star; as well as with the amount of precession and other quantities actually employed in reducing the Stars' places from their dates of observation to the date of the Catalogue.

The place of the star in the B.A.C. for 1850 is,

$$\text{R.A.} = 2^{h}\ 27^{m}\ 51^{s}\cdot35 \quad \text{and} \quad \text{N.P.D.} = 83^{\circ}\ 50'\ 0''\cdot2$$

the former being $0^{s}\cdot50$ in defect, and the latter $5''\cdot2$ in excess, according to the Edinburgh observations; and the star was

known to the compilers of the B.A. Catalogue to have a large annual proper motion, being debited by them with no less than + 0ˢ·118 in R.A., and − 1″·31 in N.P.D.

Yet these quantities were not large enough; for the Edinburgh observations show that from 1837 to 1867 the mean annual proper motion in R.A. was + 0ˢ·132, and in N.P.D. − 1″·46. Curiously enough, too, these quantities go far to justify the much earlier observations of that eminently hard-working astronomer, the late T. Glanville Taylor, who in his *Madras Catalogue* of 1835 gives the R.A. proper motion at + 0ˢ·13, and the N.P.D. at − 0″·43 per annum.

Yet the question is by no means merely removed from the B.A.C., and re-established as T. G. Taylor had placed it so many years before; for the most notable peculiarity of the Edinburgh observations is, the undoubted change that has been going on, during their period, in the amount of the proper motion; so that between 1837 and 1851 it was in R.A. at the mean annual rate of + 0ˢ·149; but between 1851 and 1867 only + 0ˢ·116.

Similarly, too, the annual proper motion in N.P.D. was between 1837 and 1852 only − 1″·42, but between 1852 and 1866 it had risen to − 1″·51, and with symptoms of being still on the increase. To illustrate, therefore, so unusual a case, I have endeavoured in the accompanying table to give, from the Edinburgh observations alone, a first approximation to the successive places of the star, and the amounts of proper motion deduced for each year, from 1837 to 1867; distinguishing all interpolated places by enclosing them in brackets.

Successive Mean Annual Places of the Star in Cetus, B.A.C. 793; *from* 1837 *to* 1867, *from Edinburgh Observations, or Interpolations of the same;* (*single observations in* 1854 57–61–67 *being omitted*).

Date of Obs.	R.A. reduced to Jan. 1 of the year of observation (or interpolation). h m s	No. of Obs.	Annual differences from Observation Curve. s	Annual Geometric Precessions computed. s	Result for Proper Motion in R.A. s
1837·90	2 27 8·84	3			
1838	(2 27 12·15)	...	+3·308	+3·156	+0·152
1839	(2 27 15·47)	...	3·312	3·156	0·156
1840	(2 27 18·79)	...	3·313	3·157	0·156
1841	(2 27 22·11)	...	3·314	3·157	0·157
1842	(2 27 25·43)	...	3·313	3·157	0·156
1843	(2 27 28·75)	...	3·313	3·157	0·156
1844	(2 27 32·06)	...	3·312	3·157	0·155
1845	(2 27 35·37)	...	3·311	3·157	0·154

Date of Obs.	R.A. reduced to Jan. 1 of the year of observation (or interpolation). h m s	No. of Obs.	Annual Differences from Observation Curve. s	Annual Geometric Precessions computed. s	Result for Proper Motion in R.A. s
1846	(2 27 38·68)	...	3·308	3·158	0·150
1847	(2 27 41·98)	...	3·305	3·158	0·147
1848	(2 27 45·28)	...	3·300	3·158	0·124
1849	(2 27 48·57)	...	3·296	3·158	0·138
1850	(2 27 51·85)	...	3·292	3·158	0·134
1851·85	2 27 55·12	5	3·286	3·158	0·128
1852·82	2 27 58·39	4	3·281	3·158	0·123
1853	(2 28 1·65)	...	3·277	3·158	0·119
1854	(2 28 4·91)	...	3·275	3·158	0·117
1855·83	2 28 8·17	5	3·273	3·159	0·114
1856	(2 28 11·44)	...	3·273	3·159	0·114
1857	(2 28 14·71)	...	3·274	3·159	0·115
1858·80	2 28 17·99	6	3·275	3·159	0·116
1859	(2 28 21·26)	...	3·276	3·159	0·117
1860	(2 28 24·53)	...	3·277	3·159	0·118
1861·86	2 28 27·80	2	3·278	3.159	0·119
1862·85	2 28 31·07	3	3·279	3·159	0·120
1863·89	2 28 34·38	3	3·280	3·160	0·120
1864	(2 28 37·67)	...	3·280	3·160	0·120
1865·85	2 28 40·96	3	3·280	3·160	0·120
1866·88	2 28 44·23	1	3·279	3·160	0.119
1867·82	2 28 47·50	3	+3·278	+3·160	+0·118

Date of Obs.	N.P.D. reduced to Jan. 1 of the year of observation (or interpolation) ° ′ ″	No. of Obs.	Annual Differences from Observation Curve. ″	Annual Geometric Precessions computed. ″	Result for Proper Motion in N.P.D. ″
1837·64	83 53 42·3	3			
1838	(83 53 24·8)	...	−17·55	−16·07	−1.48
1839	(83 53 7·3)	...	−17·55	−16·07	−1·48
1840	(83 52 49·7)	...	−17·54	−16·06	−1·48
1841	(83 52 32·1)	...	−17·54	−16·06	−1·48
1842	(83 52 14·6)	...	−17·	−16·0	−1.48

Date of Obs.	N.P.D. reduced to Jan. 1 of the year of observation (or interpolation)	No. of Obs.	Annual Differences from Observation Curve.	Annual Geometric Precessions computed.	Result for Proper Motion in N.P.D.
1843	(83° 51′ 57″·1)	...	−17″·52	−16″·05	−1″·47
1844	(83 51 39·6)	...	−17·51	−16·04	−1·47
1845	(83 51 22·1)	...	−17·50	−16·04	−1·46
1846	(83 51 4·7)	...	−17·48	−16·03	−1·45
1847	(83 50 47·3)	...	−17·47	−16·03	−1·44
1848	(85 50 29·9)	...	−17·46	−16·03	−1·43
1849	(83 50 12·5)	...	−17·45	−16·02	−1·43
1850	(83 49 55·1)	...	−17·44	−16·02	−1·42
1851	(83 49 37·7)	...	−17·44	−16·02	−1·42
1852·83	83 49 20·3	3	−17·43	−16·02	−1·41
1853	(83 49 2·9)	...	−17·42	−16·01	−1·41
1854·81	83 48 45·4	2	−17·42	−16·01	−1·41
1855	(83 48 28·1)	...	−17·43	−16·01	−1·42
1856	(83 48 10·8)	...	−17·44	−16·00	−1·44
1857	(83 47 53·5)	...	−17·46	−16·00	−1·46
1858·81	83 47 36·3	5	−17·48	−16·00	−1·48
1859	(83 47 18·7)	...	−17·50	−15·99	−1·51
1860·84	83 47 1·0	2	−17·54	−15·99	−1·55
1861	(83 46 43·6)	...	−17·58	−15·99	−1·59
1862·85	83 46 26·3	3	−17·62	−15·98	−1·64
1863·88	83 46 8·6	3	−17·64	−15·98	−1·66
1864·90	83 45 51·6	2	−17·67	−15·98	−1·69
1865·86	83 45 33·2	7	−17·70	−15·97	−17·3
1866·87	83 45 15·2	4	−17·72	−15·97	−1·75

1867

Comparisons with other Standard Catalogues.

The first question that arises on looking over the above Tabular representation of these remarkable proper motions, is, or perhaps should be—"What amount of confirmation do they receive from other Observatories?"

Unfortunately the star has not been observed much of late

anywhere; but it does happily figure in the new American Catalogue from the Washington Observatory, where all the stars are reduced to the epoch of 1860; and where many other references that I have made to it have proved it to be generally of extreme exactitude. What then was not my consternation on finding, that the Washington place for this star differed from the Edinburgh place in the same year by actually $1^{s}\cdot26$ of time in R.A., and $17''\cdot7$ of space in N.P.D.! Now had there been nothing given by the United States observers but the reduced place of the star and the epoch of their Catalogue, I fear that the Washington result would have had to be thrown overboard; but happily the astronomers there had duly entered that the date of observation was 1870·9, and that the precession they employed in bringing it up to 1860 was in R.A. + $1^{s}\cdot16$, and N.P.D. − $16''\cdot0$,—clearly showing that they had not applied any correction for any amount of proper motion whatever. Multiplying, therefore, the Edinburgh mean annual proper motions for the actual period by 10·9, and applying them to the Washington observations, I had the satisfaction of reducing the R.A. discrepancy from $1^{s}\cdot26$ to $0^{s}\cdot04$, and the N.P.D. discrepancy from $17''\cdot7$ to $0''\cdot8$.

Again the star appears, though in R.A. only, in the Second Radcliffe Catalogue of the Oxford Observatory; for 1860 also, and that Catalogue is generally a very accurate reference too. Yet, in this case, the Radcliffe place differed no less than $1^{s}\cdot71$ from the Washington Catalogue for the same year, and $0^{s}\cdot45$ from the Edinburgh observation of the period. Here again, however, the Radcliffe precession as entered showed that no proper motion had been applied; and instead of the star having been observed 10·9 years after 1860, it had been observed 2·1 years previous thereto.

Making correction accordingly for such amount of proper motion for the interval concerned, the Radcliffe difference from the Edinburgh result is reduced from $0^{s}\cdot45$ to $0^{s}\cdot20$; and if that still should seem a rather large quantity in anyone's eyes, it is probably explainable by the fact of the Radcliffe place depending on a single observation only.

On still further following up the observation-history of the star, we find it in R.A. place in the Greenwich Catalogue for 1840; and in N.P.D. place in the Greenwich Catalogue for 1845. The agreement too of each of these places with the Edinburgh places for the same times, is excellent; that is, within $0^{s}\cdot40$ for R.A. and $0''\cdot3$ for N.P.D.; and the reason is simple, viz., that the Astronomer Royal at Greenwich was aware of the large proper motion, and had applied the correction as given in the B.A. Catalogue. The quantity there is indeed, as already indicated, a trifle too small; but as the difference of time between the Greenwich observations and the dates they were respectively reduced to, for either catalogue, was only 2 years in one case and 1 in the other, such a mere residual difference did not

amount to any practically sensible quantity during such short intervals.

Altogether, this star's physical movements and human history bear out admirably an earnest remark often indulged in by the late Rev. R. Sheepshanks, to the effect, "that proper motion stars will soon require to be hunted up like planets with continual observations;" and if measure for parallax can be added thereto, we may be already on the eve of new discoveries.

15 *Royal Terrace, Edinburgh,*
1875, 1 *May.*

On the Proper Motion of B.A.C. 793. By Edwin Dunkin, Esq.

The remarkable proper motion of the star B.A.C. 793 (Piazzi II. 123), the apparent variability of which is pointed out in Professor Smyth's paper, has for some time attracted the attention of observers. In 1852 the star was placed on the working star-observing-list at the Royal Observatory, with the express object of obtaining materials for a more accurate determination of its proper motion, and between 1854 and 1858 a large number of observations were made with the transit-circle. Professor Smyth refers to places given in the Greenwich *Twelve-year Catalogue*, where the R.A. is determined from observations made with Troughton's transit-instrument in 1837 and 1838, and the N.P.D. from observations made with the mural-circle in 1845 and 1846, as a proof of accordance of the Edinburgh and Greenwich observations. But he has apparently overlooked the fact that this star appears also in the Greenwich *Six-year Catalogue*, and in the first *Seven-year Catalogue*. In the latter it is credited with the large number of 29 observations in R.A., and 30 in N.P.D., distributed in nearly equal proportion over the years 1854 to 1858.

As an addendum to Professor Smyth's paper, it will be interesting, therefore, to compare the Greenwich observed annual R.A. and N.P.D. of this star with his corresponding observed and interpolated numbers; and although the separate intervals are far too short to determine the value of the proper motion with any great accuracy, we may obtain sufficient evidence to show whether there is a tendency to a variability in the value, similar to that so clearly exhibited in the results from the Edinburgh observations. I have only to remark, that I have extracted the numbers for each year from the Greenwich annual Catalogues, and that the places given are the true observed mean places for January 1 of each year. Each yearly result has been corrected for the new value of the proper motion for the fraction

of the year. The separate years' results are contained in the following tables, the only alteration I have found necessary to make being the rejection of a transit observed on November 20, 1856, under very bad circumstances, and which is evidently erroneous :—

Year	No. of Obs.	Mean R.A. Jan. 1.	Annual Variation from Obs.	Annual Precession.	Annual Proper Motion in R.A.
		h m s	s	s	s
1837	1	2 27 9·00			
			+3·300	+3·158	+0·142
1838	4	2 27 12·30			
			+3·288	+3·158	+0·130
1852	1	2 27 58·33			
			+3·250	+3·159	+0·091
1854	4	2 28 4·83			
			+3·340	+3·159	+0·181
1855	6	2 28 8·17			
			+3·290	+3·159	+0·131
1856	5	2 28 11·46			
			+3·170	+3·160	+0·010
1857	5	2 28 14·63			
			+3·290	+3·160	+0·130
1858	8	2 28 17·92			

Year.	No. of Obs.	Mean N.P.D. Jan. 1.	Annual Variation from Obs.	Annual Precession.	Annual Proper Motion in N.P.D.
		° ′ ″	″	″	″
1845	2	83 51 23·59			
			−18·29	−16·04	−2·25
1846	3	83 51 5·30			
			−17·14	−16·03	−1·11
1852	1	83 49 22·45			
			−18·59	−16·02	−2·57
1854	4	83 48 45·27			
			−17·91	−16·01	−1·90
1855	6	83 48 27·36			
			−16·47	−16·01	−0·46
1856	6	83 48 10·89			
			−17·61	−16·01	−1·60
1857	6	83 47 53·28			
			−16·80	−16·00	−0·80
1858	8	83 47 36·48			

Although the numbers in the last columns differ *inter se* more than we might expect, considering that the results, with one or two exceptions, depend on a fair number of observations, yet most of the relative discordances are undoubtedly owing to errors of observation. Unfortunately, no observations of this star have been made at Greenwich since 1858, and it is since that time that the proper motion in N.P.D., according to the Edinburgh observations, has so much increased; but a comparison of the corresponding results deduced from the observations made at Edinburgh and Greenwich from 1837 to 1858, confirms in some measure the accuracy of the numbers contained in Professor Smyth's table, both observed and interpolated. This evidence is, however, too slight to speak with certainty as to the reality of the

variability of the proper motion; but if we divide the interval between the first and the last Greenwich observations into three short intervals, a very marked agreement is shown in the three results for proper motion in R.A., as determined from the two independent series of observations. The change during this period in the proper motion in N.P.D., as given in the Edinburgh results, is very slight, and is far too small for us to expect more in this element than a general agreement. The following are the proper motions in R.A. and N.P.D., deduced directly from the numbers in the corresponding tables in Professor Smyth's paper and in this Note :—

Interval.	Proper Motion in R.A.		Proper Motion in N.P.D.	
	Edinburgh.	Greenwich.	Edinburgh.	Greenwich.
	s	s	"	"
1837 to 1838	+0·152	+0·142	...	...
1838 to 1852	+0·144	+0·130	...	...
1845 to 1846	...	...	−1·36	−2·25
1846 to 1852	...	...	−1·37	−1·11
1852 to 1858	+0·108	+0·106	−1·32	−1·65

The above numbers sufficiently show the importance of continuing the observations of this star; it is therefore again inserted in the Greenwich working-catalogue, and probably a series of observations of it will be made before the close of the year. That its proper motion in both elements is a remarkable one, an inspection of Professor Smyth's table is sufficient proof; and it is for this reason that I have thought it interesting to insert in the same number of the *Monthly Notices* the results from the Greenwich observations of this star as a supplement to Professor Smyth's paper.

Kenwyn, Kidbrooke,
Blackheath, 1875, *May* 10.

On the Period of the Double Star η Coronæ Borealis.

By J. M. Wilson, Esq.

The period of this star seems to be not yet quite satisfactorily ascertained in spite of the very great care with which its orbit has been studied. Herschel in 1833 first calculated the orbit, and made its period 44·242 years. Mädler computed three orbits. In 1838 he made the period 43·310; in 1842, 43·246; in 1847, 42·500 years, respectively. Villarceau produced two orbits on different hypotheses as to an early observation of Sir W. Herschel, one giving a period of 42·501 years, the other, which he preferred, but which events have declared against, of 67·309 years.

Winnecke, using observations up to the spring of 1856, made a very careful examination of the orbit in his *De Stellâ η Coronæ Borealis Duplici Dissertatio Astronomica Inauguralis.* At this time there appeared to be ample materials for calculating an orbit, for the star had been well watched through 300° of its orbit, independently of two early isolated measures of Sir W. Herschel, in 1781·69, and 1802·69, which might be valuable as affording confirmation of the period. After the most complete discussion, he decides on an orbit with a period of 43·115 years, "cujus comparatio cum observationibus nobis demonstravit reddere id observationes usque ad quantitates quas in mensuris ipsis præstare non possumus." He says also elsewhere, that these elements "reddunt observationes ipsissimas;" and his comparison of the calculated and observed places justifies this remark.

And yet his orbit appears now to be considerably in error. For recent observations, as far as I have been able to procure them, are as follow:—

Table I.

t	θ	Observer.	No. of nights.
1863·03	19°·04	Dembowski	24
64·43	24·16	„	10
65·44	27·52	Dawes	3
·49	27·40	Dembowski	9
66·44	30·06	„	9
67·50	33·19	„	7
68·39	36·47	„	7
70·38	44·01	„	8
·4	44·6	Gledhill	1
·45	44·15	Talmage	
71·41	47·70	„	
·45	47·72	Dembowski	8
·5	46·47	Gledhill	2
·56	47·60	Seabroke	2
72·29	47·83	Talmage	
·49	51·	Wilson and Seabroke	1
73·41	55·90	„	5
·55	55·45	Gledhill	2
74·34	58·43	„	2
·47	58·25	Wilson and Seabroke	2
75·31	68·10	Seabroke and Smith	3

By means of these I have constructed a table of interpolated positions for the beginning of each year; which is compared in

the following table with the positions obtained from Winnecke's orbit, showing a systematic and increasing divergence, which is far too large to be accidental.

Table 2.

t	θ Obs.	θ' Calc.	$\theta' - \theta$
1863	18°·9	16°·8	− 2·1
64	22·3	19·8	− 2·5
65	25·6	22·6	− 3·0
66	28·8	25·3	− 3·5
67	31·9	27·9	− 4·0
68	35·	30·5	− 4·5
69	38·3	33·2	− 5·1
70	41·9	35·9	− 6·0
71	45·7	38·8	− 6·9
72	49·8	41·9	− 7·9
73	54·2	45·3	− 8·9
74	58·9	49·0	− 9·9
75	64·2	53·2	−11·

It may be remarked that the star is at its maximum distance in the year 1866 or 1867, a distance slightly greater than 1″·1, and in the years 1873 and 1874 the distance is very nearly 1″, and that therefore a systematic error of observation of the amount stated is impossible.

In order to ascertain the period, we may compare the observations during its last revolution when it was in the same part of its orbit that it now occupies. They are, corrected for precession to the year 1880, as follow:—

Table 3.

t	θ	Observer	No. of nights
1831·34	50°·23	Dawes	2
·42	52·27	Herschel	10
·63	50·60	Struve	3
32·48	56·48	Herschel	10
·55	56·48	Dawes	1
·76	56·65	Struve	3
33·28	61·88	Herschel	8
·39	63·32	Dawes	3
35·41	74·07	Struve	6
36·52	88·49	Struve	6
37·47	95·06	Struve	4
38·44	106·80	Struve	5

These observations, which are not very accordant, give as interpolated values (subject to some error), in a more convenient form for comparison of dates, and calculation of the period, the following table:—

Table 4.

θ	t	t'
50°	1830·91	72·1
55	32·00	73·2
60	33·03	74·2
65	33·80	75·0
70	34·49	
75	35·13	
80	35·75	
85	36·33	
90	36·87	
95	37·37	
100	37·84	
105	38·27	
110	38·66	

So far as these suffice to give a sidereal period they give one of 41·2 years.

It will be interesting to examine how far this period satisfies the early observations of Sir W. Herschel. The comparison is as follows:—

t	θ obs.	θ' calc.	$\theta'-\theta$
1781·69	30°·35	22°·9	−7°·4
1802·69	180·11	180·0	−0·1

By Winnecke's period the corresponding errors are − 0°·04 and + 6°·89 respectively, which are nearly as large.

The hypothesis that would best satisfy the observations is, that there exists in each successive revolution some shortening of the period, accompanied perhaps with a progression of the line of apsides. But it is too soon to attempt to submit this to a strict examination. It is at any rate demonstrated that the period of the star is shorter than any that has been hitherto assigned to it, and that it does not differ widely from 41·2 years. A reference to Table 4 during the next five years will perhaps elicit some remarkable results.

Temple Observatory, Rugby,
1875, *April.*

Observations and Orbit of the Double Star Σ 1728=42 Comæ Berenices. By M. Otto Struve.

This double star, discovered by my father in 1826, began to attract his particular attention since 1833, when it offered for the first time the appearance of a single star. Since 1833 it has been observed as regularly as possible every year by my father at Dorpat and by myself at Pulkowa, and during the last 40 years has presented three times more the rare phenomenon of an occultation of one star by the other. I beg leave to communicate to the Society the complete list of annual means of our observations on this remarkable system, together with the conclusions to which I have been led lately concerning the orbit. My observations, it must be remarked, are corrected throughout for the effect of systematic errors, determined by measures of artificial double stars; I have also applied to the distances estimated by my father the corrections deduced by himself in a similar way (*Mens. Micr.* p. cliv.).

Dorpat Observations.

	″	°	day
1827·28	e = 0·570	P = 10.9	1
29·40	0·640	11·6	3
33·36	Susp. fig. obs.	350·7	2
34·43	... ...	228·3	1
35·39	Oblonga	191·2	4
36·42	0·430	190·2	3
37·40	0·515	190·8	6
38·40	0·483	191·5	3

Pulkowa Observations.

	″	°	day
40·45	0·550	195·7	3
41·41	0·495	194·5	2
42·40	0·318	193·9	3
45·47	Simplex	...	1
46·40	Susp. fig. obs.	67·0	3
47·42	0·200	15·5	3
48·42	0·267	12·7	3
49·42	0·420	8·6	3
50·39	0·480	11·4	3
51·42	0·488	7·0	4
52·43	0·563	10·9	3
53·40	0·570	10·8	3
54·38	0·600	14·1	1

	′	°	day
55·44	0·620	9·1	2
57·49	0·435	7·7	2
58·44	0·385	8·5	2
59·37	Susp. fig. obs.	4·0	4
61·42	0·435	185·6	2
62·40	0·545	191·6	2
63·44	0·550	189·3	1
64·42	0·513	192·5	3
66·44	0·403	188·5	3
67·47	0·365	193·0	2
68·44	0·215	195·8	2
69·47	Susp. fig. obs.	195·0	1
70·44	Simplex	...	2
71·43	Simplex	...	1
72·42	Oblonga	20·0	1
73·46	0·200	9·0	2
74·41	0·302	9·2	2

The two stars are so very nearly of the same brightness (both 6·0 according to my father) that I never could make out with certainty which of the two was the brighter. This circumstance and the apparent rectilinear motion left, even after three occultations, still some doubts concerning the period of revolution, which could be supposed either of about 13 years or of double that time. The recent observations have decided in favour of the second hypothesis, and thereby I have been enabled to distinguish between the two branches of the orbit, as it is done in the foregoing list by the difference of 180° in the indicated directions. The quite insignificant variations in the observed directions in all instances—when the two stars were seen separated or offered together a distinctly elongated figure—show clearly that the plane of the orbit coincides so nearly with the direction to the Sun that we have not the least trace of a sensible inclination. Consequently the situation of the plane is exactly determined. We have to adopt the inclination $i=90°$, and the angle between the ascending node and the declination circle equal to the mean of all observed directions $=11°{\cdot}0$.

Thus the number of elements to be deduced from our observations is reduced to five. But this simplification of the problem is more than outweighed by the unfavourable condition, that for the deduction of those five elements we cannot make any use of the observed directions, but must rely entirely upon the measured and estimated distances.

Already in 1866 (*Bulletin de l'Académie de St. Pétersbourg*, t. iii.) I have stated the reasons which led me to the conclusion that the period of revolution ought to be supposed about 25·5

years. This conclusion was then principally based upon the remark that between the two first occultations (1834 and 1845) there had elapsed only 11-12 years; while the next following occultation (1859) happened after an interval of rather more than 14 years. This supposition is completely confirmed by the more recent observations, which show that in correspondence with the first period the latest occultation (1870-71) has happened again after about 11 years. Of course, if there is such an alternation in the periods of visibility, two of them will together form an entire period of revolution. But there is now one feature more come out, which, in corroborating these conclusions, must at once appear most useful for a more accurate deduction of all the other elements. We see that about the time of the two corresponding occultations (1845 and 1871) five years have elapsed between the epochs when the two stars ceased to appear separated till they began to separate again, or between distances from about $+0''\cdot2$ to $-0''\cdot2$, while at the time of the intermediate occultation (1859) three years were sufficient to produce a change of from $0''\cdot4$ in the decrease to $0''\cdot4$ in the increase. The occultation of 1834, corresponding to that of 1859, shows likewise a comparatively rapid transition from the phase of simplicity to larger distances. This remark, combined with the difference in the intervals of the occultations, might necessarily lead us to the conclusion that the real orbit of the system is considerably excentrical and its major axis not much inclined to the direction in which we see the stars.

The general figure of the orbit being thus fixed, we proceeded to express it in numerical values; and after some trials, found that the following elements would tolerably well represent all measured and estimated distances:—

Time of passage through periaster	$T = 1860\cdot0$
Angle in the orbit between periaster and ascending node	$\lambda = 100^\circ$
Major semi-axis	$a = 0''\cdot693$
Excentricity	$e = 0\cdot50$
Mean annual motion	$m = 13^\circ51'$ or revolution $= 26^{y}\cdot0$.

Starting from these elements, one of my assistants, M. Dubiago, developed the differential formulæ for distances, and proceeded to the inquiry by what elements, according to the method of least squares, all observations would be best represented. For this purpose it seemed desirable to make up, not only of those observations in which the distance of the two centres had been immediately measured or estimated, but also of those in which, on account of their closeness, the distances had to be derived from the general appearance produced by the two stars together. Accordingly, I examined carefully the expressions used on those occasions in the original manuscripts. This examination, together with the experience won by numerous

other observations, engaged me to fix upon the following distances as the most probable :—

1845·47	70·44	71·44		d = 0″·00
33·36	46·40			= 0·10
34·43	59·37	69·47	72·42	= 0·15
35·39				= 0·25

These estimations can hardly be subject to errors of more than a few hundredth figures of the second decimal, as the whole amount does not exceed 0″·25, and therefore may be regarded as of nearly the same weight as the direct measures and estimations. Then, again, in the list of immediately estimated distances there were two to which I proposed to attribute only half the weight of that of the other observations. These were the observations of 1840 and 1841. It is a curious and not yet sufficiently explained fact, that all distances of double stars measured and estimated by me during the first two years of my residence at Pulkowa, come out by 0″·1 to 0″·2 greater than those observed by my father at Dorpat, and likewise than those observed by me since 1842 at Pulkowa, for which I have determined the constant and systematic errors by means of artificial double stars. The exact value of that constant difference will result from a general discussion of all my micrometrical measures, which I hope I shall soon be able to submit to the astronomical public. But before this is done, I thought it best for the present research to leave these earlier observations unaltered, but at present to give them only half the weight.

Thus our observations offered M. Dubiago 38 equations of condition. Having resolved them by the method of least squares, he found the following elements :—

$$T = 1869{\cdot}92 \pm 0^{y}{\cdot}080$$
$$\lambda = 99^{\circ}{\cdot}11' \pm 0^{\circ}{\cdot}45^{s}{\cdot}6$$
$$a = 0''{\cdot}657 \pm 0''{\cdot}0126$$
$$e = 0{\cdot}480 \pm 0{\cdot}0239$$
$$m = 14^{\circ}0'{\cdot}2 \pm 2'{\cdot}75, \text{ or revolution} = 25^{y}{\cdot}71 \pm 0^{y}{\cdot}084$$

With these elements M. Dubiago computed directly the distances, and got the following differences between observation and computation :—

	O−C		O−C
	″		″
1827·28	−0·023	1853.40	−0·038
29·40	+0·014	54·38	−0·028
33·36	−0·070	55·44	0·000
34·43	+0·037	57·49	−0·036
35·39	−0·078	58·44	+0·072
36·42	−0·034	59·37	+0·057

	O−C		O−C
	"		"
37·40	−0·006	61·42	+0·053
38·40	−0·041	62·40	+0·057
40·45	+0·116	63·44	+0·023
41·41	+0·130	64·42	−0·004
42·40	+0·036	66·44	−0·012
45·47	−0·003	67·47	+0·028
46·40	+0·008	68·44	−0·039
47·42	+0·011	69·47	−0·010
48·42	−0·012	70·44	−0·063
49·42	+0·055	71·43	−0·027
50·39	+0·039	72·42	+0·028
51·42	−0·024	73·46	−0·019
52·43	−0·005	74·41	−0·002

From these differences we deduce the probable error of the single annual means = 0″·030, or that of a single observed distance = 0″·046, certainly a very satisfactory result. The only two distances that differ more than 0″·1 from the computation are the two prospected of the years 1840 and 1841; the sign of their differences and their amount agrees completely with what is indicated by numerous other comparisons. If we had rejected entirely those two distances, the probable error would have been found considerably less; and probably also the general distribution of the sign of the differences would have been yet more satisfactory than it is now.

The elements having been deduced exclusively from my father's and my own observations, it was interesting to examine how the observations of other astronomers would agree with them. For this purpose M. Dubiago has compared also the distances observed by the late Mr. Dawes and by M. Secchi.

Observations of Mr. Dawes.

	Distance.			
	Obs.	Comp.	days	O−C
	"	"		"
1840·74	0·425 est.	0·414	3	+0·011
42·53	Simplex	0·272	1	(−0·272)
43·45	Simplex	0·189	1	(−0·189)
53·09	0·626	0·596	4	+0·030
54·39	0·552	0·628	5	−0·076
60·34	0·2 est.	0·108	1	+0·092
63·25	0·8 est.	0·524	1	−0·024
64·43	0·45 est.	0·517	1	−0·067

Observations of M. Secchi.

	″ Obs.	″ Comp.	days	″ O−C
1856·35	0·45 est.	0·580	2	−0·130
57·37	0·486	0·486	3	0·000
58·39	0·3 est.	0·323	1	−0·023

Evidently for both observers the agreement is likewise quite satisfactory. There is only Dawes's observation of 1842, for which the difference is greater than might be expected. In fact, it might appear strange that Dawes, with his excellent eye, should have said that year the star appeared to him quite round, if the real distance of the centre had been 0″·272, but all conclusions on this account lose their power if we consider that in that year he had only one evening's observations, and that he says expressively that on this occasion the air was not good. The power used by him that night was only of 420, hardly sufficient to show distinctly such small quantities, if the state of the atmosphere was not first rate.

With regard to the directions, the agreement of the different observers is as perfect as can be desired. If we exclude those observations for which the closeness of the two stars did not admit of a direct estimation or measure of the distance, the means for the individual observers are:—

	°
W. Struve P	= 10·9
O. Struve	= 11·1
Dawes	= 12·2
Secchi	= 12·4
Dembowski	= 9·6

and the single observations deviate from these means only of quantities that can be regarded as quite evanescent even at the maximum distance of 0″·65. Future observations, by still more perfect instruments, will, perhaps, decide if the larger discrepancies from those means, indicated by my father and myself at epochs (1834, 1846, and 1859) when we were hardly able to discern a deviation from the circular form, were real or not. A very small error in the centration of the object-glass, so small that its existence would hardly be suspected with single stars, might, on these occasions, have been sufficient to modify considerably our judgment on the directions in which the image appeared elongated. We see also that, amongst the three mentioned observations, that of 1859 is in manifest contradiction with the two others. If the angle observed by my father in 1834, shortly after occultation, was really larger in the middle direction, then it ought to have been found smaller in 1859, half a year before occultation, or vice versâ. Therefore, we can say

that the supposition of a perfect coincidence of the ray of vision with the orbit is as exact as the observations admit of.

The small probable errors deduced by M. Dubiago show that the elements are probably the most accurate of all that until now have been computed for orbits of double stars. This result is greatly due to the favourable circumstance that in this case we could dispose of nearly two complete revolutions. But it is also a remarkable fact that these elements rest entirely upon distances; which until now have acted only a very subordinate part in the determination of similar orbits.

Pulkowa, 1875, *April*.

Elements of Binary Stars. By Dr. W. Doberck.

μ^2 *Boötis.*

Node = 182° 59′
γ = 44° 26′
λ = ∤ 17° 41′
e = 0·6174
P = 290·07 yrs.
T = 1863·51
a = 1″·500.

b Coronæ.

Node = 6° 43′
γ = 29° 40′
λ = 89° 17′
e = 0·7502
P = 843·2 yrs.
T = 1828·91
a = 6″·001.

Col. Cooper's Observatory,
1875, *May*.

On the Star 61 Geminorum. By Geo. Knott, Esq.

In a Note on the star 61 *Geminorum* in the April number of the *Monthly Notices*, the Rev. T. W. Webb calls attention to the recovery, by H. Sadler, Esq., of Honiton, of Admiral Smyth's *comes*, which had, on several occasions, been looked for unsuccessfully by himself and by me. I think, however, that a comparison of the position-angle of the star observed by Mr. Sadler with that assigned by Admiral Smyth to the star observed by him, throws some doubt on the supposed identity of the two objects,—the position-angle of the Bedford Catalogue being 110°·0, while that of Mr. Sadler's star is 160° or 165°—a difference of some 50° or 55°.

I searched for Admiral Smyth's *comes* on the 2nd and 26th February, 1861, with my $7\frac{1}{3}$-inch Alvan Clark refractor, but could see no star in the place indicated by him. I noted the following stars in the field:—A star of 10 mag., position-angle 61°·6, distance 321″, with a small companion to the *n.f.*:—A star of 12 mag., position-angle 170° or 175°, distance 75″, which agrees fairly with the place of the star observed by Mr. Sadler; and a small star at approximately the same distance in the north vertical. These two faint *comites* I again noted on a re-examination of the object on the 20th December, 1871, when I again failed to see Admiral Smyth's star.

Cuckfield, 1875, *May* 13.

Errata.

"Observations of the Satellites of *Saturn*."

p. 328 ff. 7th column, read, "distance", instead of "Pos.-Ang."
„ "r" or "rev." „ "°",
the apparent distance being expressed in revolutions of the micrometer.
p. 329, *Rhea*, Oct. 17, 4th col., read "4" instead of "2".
p. 330, *Titan*, Aug. 31, 3rd col. „ "258·80" „ "238·80".
Oct. 15 „ „ "138·9" „ "138·0".

"Bessel's Investigation of the Refraction through an Atmosphere."

Equation 4, p. 332. The factor $a^2 g_o$ should precede $\frac{\delta_o}{p_o}$; and in the equation before (4), a^2 should precede g_o, whilst the value of β should be $g_o \frac{\delta_o}{p_o} a$.

Page 336, line 6 from bottom. A semicolon should follow the word "favourable", and for "ensuring" read "assuming."